ON THE

PROBABLE SEAT

OF

VOLCANIC ACTION.

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The igneous theory of the earth's crust, which supposes it to have been at one time a fused mass, and to still retain in its interior a great degree of heat, is now generally admitted. In order to explain the origin of eruptive rocks, the phenomena of volcanos, and the movements of the earth's crust, all of which are conceived by geologists to depend upon the internal heat of the earth, three principal hypotheses have been put forward. Of these the first supposes that in the cooling of the globe a solid crust of no great thickness was formed, which rests upon the still uncongealed nucleus. The second hypothesis, maintained by Hopkins and by Poulett Scrope, supposes solidification to have commenced at the centre of the liquid globe, and to have advanced towards the circumference. Before the last portions became solidified, there was produced, it is conceived, a condition of imperfect liquidity, preventing the sinking of the cooled and heavier particles, and giving rise to a superficial crust, from which solidification would proceed downwards. There would thus be enclosed, between the inner and outer solid parts, a portion of uncongealed matter, which, according to Hopkins, may be supposed still to retain its liquid condition, and to be the seat of volcanic action, whether existing in isolated reservoirs or subterranean lakes; or whether, as suggested by Scrope, forming a continuous sheet surrounding the solid nucleus, whose existence is thus conciliated with the evident facts of a flexible crust, and of liquid ignited matters beneath.

Hopkins, in the discussion of this question, insisted upon the fact, established by his experiments, that pressure favors the

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solidification of matters which, like rocks, pass in melting to a less dense condition, and hence concludes that the pressure existing at great depths must have induced solidification of the molten mass at a temperature at which, under a less pressure, it would have remained liquid. Mr. Scrope has followed this up by the ingenious suggestion that the great pressure upon parts of the solid igneous mass may become relaxed from the effect of local movements of the earth's crust, causing portions of the solidified matter to pass immediately into the liquid state, thus giving rise to eruptive rocks in regions where all before was solid.*

Similar views have been put forward in a note by Rev. O. Fisher, and in an essay on the formation of mountain chains, by Mr. N. S. Shaler, in the proceedings of the Boston Society of Natural History, both of which appear in the *Geological Magazine* for November last. As summed up by Mr. Shaler, the second hypothesis supposes that the earth "consists of an immense solid nucleus, a hardened outer crust, and an intermediate region of comparatively slight depth, in an imperfect state of igneous fusion." In this connection it is curious to remark that, as pointed out by Mr. J. Clifton Ward, in the same Magazine for December (page 581), Halley was led, from the study of terrestrial magnetism, to a similar hypothesis. He supposed the existence of two magnetic poles situated in the earth's outer crust, and two others in an interior mass, separated from the solid envelope by a fluid medium, and revolving, by a very small degree, slower than the outer crust.† The same conclusion was subsequently adopted by Hansteen.

The formation of a solid layer at the surface of the viscid and nearly congealed mass of the cooling globe, as supposed by the advocates of the second hypothesis, is readily admissible. That this process should commence when the remaining envelope of

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* See Scrope on Volcanos, and his communication to the *Geological Magazine* for Dec., 1868.

† The elevated temperature of the interior of the globe would probably offer no obstacle to the development of magnetism. In a recent experiment of M. Trève, communicated by M. Faye to the French Academy of Sciences, it was found that molten cast iron when poured into a mould, surrounded by a helix which was traversed by an electric current, became a strong magnet when liquid at a temperature of 1300° C., and retained its magnetism while cooling (Comptes Rendus de l'Acad. des Sciences, Feb., 1869.)

liquid was yet so deep that the refrigeration from that time to the present has not been sufficient for its entire solidification, is, however, not so probable. Such a crust on the cooling superficial layer would, from the contraction consequent on the further refrigeration of the liquid stratum beneath, become more or less depressed and corrugated, so that there would probably result, as I have elsewhere said, "an irregular diversified surface from the contraction of the congealing mass, which at last formed a liquid bath of no great depth, surrounding the solid nucleus." Geological phenomena do not, however, in my opinion, afford any evidence of the existence of yet unsolidified portions of the originally liquid material, but are more simply explained by the third hypothesis. This, like the last, supposes the existence of a solid nucleus, and of an outer crust, with an interposed layer of partially fluid matter, which is not, however, a still unsolidified portion of the once liquid globe, but consists of the outer part of the congealed primitive mass, disintegrated and modified by chemical and mechanical agencies, impregnated with water, and in a state of igneo-aqueous fusion.

The history of this view forms an interesting chapter in geology. As remarked by Humboldt, a notion that volcanic phenomena have their seat in the sedimentary formations, and are dependent on the combustion of organic substances, belongs to the infancy of geology. To this period belong the theories of Lémery and Breislak (*Cosmos*, v. 443; Otte's translation). Keferstein in his *Naturgeschichte des Erdkörpers*, published in 1834, maintained that all crystalline non-stratified rocks, from granite to lava, are products of the transformation of sedimentary strata, in part very recent, and that there is no well-defined line to be drawn between Neptunian and volcanic rocks, since they pass into each other. Volcanic phenomena, according to him, have their origin not in an igneous fluid centre, nor in an oxydizing metallic nucleus (Davy, Daubeny), but in known sedimentary formations, where they are the result of a peculiar kind of fermentation, which crystallizes and arranges in new forms the elements of the sedimentary strata, with an evolution of heat as a result of the chemical process (*Naturgeschichte*, vol. i. p. 109; also *Bull. Soc. Geol. de France* [1], vol. vii. p. 197). In commenting upon these views (*Am. Jour. Science*, July, 1860), I have remarked that, by ignoring the incandescent nucleus as a source of heat, Keferstein has excluded the true

exciting cause of the chemical changes which take place in the buried sediments. The notion of a subterranean combustion or fermentation, as a source of heat, is to be rejected as irrational.

A view identical with that of Keferstein, as to the seat of volcanic phenomena, was soon after put forth by Sir John Herschel, in a letter to Sir Charles Lyell, in 1836 (*Proc. Geol. Soc. London*, ii. 548.) Starting from the suggestion of Scrope and Babbage, that the isothermal horizons in the earth's crust must rise as a consequence of the accumulation of sediments, he insisted that deeply buried strata will thus become crystallized by heat, and may eventually, with their included water, be raised to the melting point, by which process gases would be generated, and earthquakes and volcanic eruptions follow. At the same time the mechanical disturbance of the equilibrium of pressure, consequent upon a transfer of sediments, while the yielding surface reposes on matters partly liquified, will explain the movements of elevation and subsidence of the earth's crust. Herschel was probably ignorant of the extent to which his views had been anticipated by Keferstein; and the suggestions of the one and the other seemed to have passed unnoticed by geologists until, in March, 1858, I reproduced them in a paper read before the Canadian Institute (Toronto,) being at that time acquainted with Herschel's letter, but not having met with the writings of Keferstein. I there considered the reaction which would take place under the influence of a high temperature in sediments permeated with water, and containing, besides silicious and aluminous matter, carbonates, sulphates, chlorids, and carbonaceous substances. From these, it was shown, might be produced all the gaseous emanations of volcanic districts, while from aqueo-igneous fusion of the various admixtures might result the great variety of eruptive rocks. To quote the words of my paper just referred to: "We conceive that the earth's solid crust of anhydrous and primitive igneous rock is everywhere deeply concealed beneath its own ruins, which form a great mass of sedimentary strata, permeated by water. As heat from beneath invades these sediments, it produces in them that change which constitutes normal metamorphism. These rocks, at a sufficient depth, are necessarily in a state of igneo-aqueous fusion; and in the event of fracture in the overlying strata, may rise among them, taking the form of to eruptive rocks. When the nature of the sediments is such as

generate great amounts of elastic fluids by their fusion, earthquakes and volcanic eruptions may result, and these—other things being equal—will be most likely to occur under the more recent formations." (*Canadian Journal*, May, 1858, vol. iii. p. 207.)

The same views are insisted upon in a paper "On some Points in Chemical Geology" (Quart. Jour. Geol. Soc., London, Nov. 1859, vol. xv. page 594,) and have since been repeatedly put forward by me, with farther explanations as to what I have designated above, *the ruins of the crust of anhydrous and primitive igneous rock*. This, it is conceived, must, by contraction in cooling, have become porous and permeable, for a considerable depth, to the waters afterwards precipitated upon its surface. In this way it was prepared alike for mechanical disintegration, and for the chemical action of the acids, which, as shown in the two papers just referred to, must have been present in the air and the waters of the time. It is, moreover, not improbable that a yet unsolidified sheet of molten matter may then have existed beneath the earth's crust, and may have intervened in the volcanic phenomena of that early period, contributing, by its extravasation, to swell the vast amount of mineral matter then brought within aqueous and atmospheric influences. The earth, air, and water thus made to react upon each other, constitute the first matter from which, by mechanical and chemical transformations, the whole mineral world known to us has been produced.

It is the lower portions of this great disintegrated and water-impregnated mass which form, according to the present hypothesis, the semi-liquid layer supposed to intervene between the outer solid crust and the inner solid and anhydrous nucleus. In order to obtain a correct notion of the condition of this mass, both in earlier and later times, two points must be especially considered, the relation of temperature to depth, and that of solubility to pressure. It being conceded that the increase of temperature in descending in the earth's crust is due to the transmission and escape of heat from the interior, Mr. Hopkins showed mathematically that there exists a constant proportion between the effect of internal heat at the surface and the rate at which the temperature increases in descending. Thus, at the present time, while the mean temperature at the earth's surface is augmented only about one-twentieth of a degree Fahrenheit, by the escape of heat from below, the increase is to be found to be equal to

about one degree for each sixty feet in depth. If, however, we go back to a period in the history of our globe when the heat passing upwards through its crust was sufficient to raise the superficial temperature twenty times as much as at present, that is to say, one degree of Fahrenheit, the augmentation of heat in descending would be twenty times as great as now, or one degree for each three feet in depth (Geol. Journal, viii. 59.) The conclusion is inevitable that a condition of things must have existed during long peri[illegible] in the history of the cooling globe when the accumulation of comparatively thin layers of sediment would have been sufficient to give rise to all the phenomena of metamorphism, vulcanicity, and movements of the crust, whose origin Herschel has so well explained.

Coming, in the next place, to consider the influence of pressure upon the buried materials derived from the mechanical and chemical disintegration of the primitive crust, we find that by the presence of heated water throughout them, they are placed under conditions very unlike those of the original cooling mass. While pressure raises the fusing point of such bodies as expand in passing into the liquid state. it depresses that point for those which, like ice, contract in becoming liquid. The same principle extends to that liquefaction which constitutes solution; where, as is with few exceptions the case, the process is attended with condensation or diminution of volume, pressure will, as shewn by the experiments of Sorby, augment the solvent power of the liquid.* Under the influence of the elevated temperature, and the great pressure which prevail at considerable depths, sediments should, therefore, by the effect of the water which they contain, acquire a certain degree of liquidity, rendering not improbable the suggestion of Scheerer, that the presence of five or ten per cent. of water may suffice, at temperatures approaching redness, to give to a granitic mass a liquidity partaking at once of the character of an igneous and an aqueous fusion. The studies by Mr. Sorby of the cavities in crystals have led him to conclude that the constituents of granitic and trachytic rocks have crystallized in the presence of liquid water, under great pressure, at temperatures not above redness, and consequently very far below that required for simple igneous fusion. The intervention of water in giving liquidity to lavas, has,

* Sorby, Bakerian Lecture, Royal Society, 1863.

in fact, long been taught by Scrope, and notwithstanding the opposition of Plutonists, like Durocher, Fournet, and Rivière, is now very generally admitted. In this connection, the reader is referred to the *Geological Magazine* for February, 1868, page 57, where the history of this question is discussed.

It may here be remarked that if we regard the liquefaction of heated rocks under great pressure, and in presence of water, as a process of solution rather than of fusion, it would follow that diminution of pressure, as supposed by Mr. Scrope, would cause not liquefaction, but the reverse. The mechanical pressure of great accumulations of sediment is to be regarded as co-operating with heat to augment the solvent action of the water, and as being thus one of the efficient causes of the liquefaction of deeply buried sedimentary rocks.

That water intervenes not only in the phenomena of volcanic eruptions, but in the crystallization of the minerals of eruptive rocks, which have been formed at temperatures far below that of igneous fusion, is a fact not easily reconciled with either the first or the second hypothesis of volcanic action, but is in perfect accordance with the one here maintained, which is also strongly supported by the study of the chemical composition of igneous rocks. These are generally referred to two great divisions, corresponding to what have been designated the trachytic and pyroxenic types, and to account for their origin, a separation of a liquid igneous mass beneath the earth's crust into two layers of acid and basic silicates, was imagined by Phillips, Durocher, and Bunsen. The latter, as is well known, has calculated the normal composition of these supposed trachytic and pyroxenic magmas, and conceives that from them, either separately, or by admixture, the various eruptive rocks are derived; so that the amounts of alumina, lime, magnesia, and alkalies, sustain a constant relation to the silica in the rock. If, however, we examine the analyses of the eruptive rocks in Hungary and Armenia, made by Streng, and put forward in support of this view, there will be found such discrepancies between the actual and the calculated results as to throw grave doubts on Bunsen's hypothesis.

Two things become apparent from a study of the chemical nature of eruptive rocks; first, that their composition presents such variations as are irreconcilable with the simple origin generally assigned to them, and second, that it is similar to that of

sedimentary rocks whose history and origin it is, in most cases, not difficult to trace. I have elsewhere pointed out how the natural operation of mechanical and chemical agencies tends to produce among sediments, a separation into two classes, corresponding to the two great divisions above noticed. From the mode of their accumulation, however, great variations must exist in the composition of the sediments, corresponding to many of the varieties presented by eruptive rocks. The careful study of stratified rocks of aqueous origin discloses, in addition to these, the existence of deposits of basic silicates of peculiar types. Some of these are in great part magnesian, others consist of compounds like anorthite and labradorite, highly aluminous basic silicates, in which lime and soda enter to the almost complete exclusion of magnesia and other bases; while in the masses of pinite or agalmatolite rock we have a similar aluminous silicate, in which lime and magnesia are wanting, and potash is the predominant alkali. In such sediments as these just enumerated we find the representatives of eruptive rocks like peridotite, phonolite, leucitophyre, and similar rocks, which are so many exceptions in the basic group of Bunsen. As, however, they are represented in the sediments of the earth's crust, their appearance as exotic rocks, consequent upon a softening and extravasation of the more easily liquefiable strata of deeply buried formations, is readily and simply explained.*

The object of the present communication has been to call the attention of geologists to the neglected views of Keferstein and Herschel, which I have endeavoured to extend and to adapt to the present state of our knowledge. It is proposed in another paper to consider the question of the agencies which have regulated the geographical distribution of volcanic phenomena both in ancient and in modern times.

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* See in this connection the *Canadian Journal* for 1858, p. 203; Quart. Geo. Society for 1859, p. 494; Amer. Jour. Science [2] xxxvii., 255, xxxviii. 182; also Geology of Canada, 1863, pp. 643, 669, and Rep. Geol. Canada, 1866, p. 230.